

# Handlon, Joseph H. 2002

## Dr. Joseph H. Handlon Oral History 2002

Download the PDF: [Handlon\\_Joseph\\_Oral\\_History\\_2002](#) (PDF 80 kB)

Joseph H. Handlon Interview  
May 14, 2002  
Interviewer: Ingrid Farreras

This is an interview with Dr. Joseph Handlon, former member of the Personality Section of the Laboratory of Psychology of the NIMH Intramural Program, Bethesda, Maryland. Served at NIMH: summer '55, 1957-1961 The interviewer is Dr. Ingrid Farreras of the NIH History Office.

Farreras: Why don't we start with some background information before we jump into the NIMH years. Could you tell me a little bit about your family background, where you were born, that sort of thing?

Handlon: Sure, I'd be happy to start with that. That will be important because I think that obviously has an influence. I was born in San Francisco, California, and I'm an only child of an only child – my mother. She was born here in Santa Barbara. My dad was born in England but moved to New Jersey when he was nine. He came West to San Francisco when he was a young man, in the early days, about 1902. There were no immediate families around me, so I didn't have any cousins or aunts or uncles – just grandparents in Santa Barbara. So, I really grew up with a very different kind of background. I was also seen as a sickly kid. I was always coming down with colds, asthma, bronchitis, etc. What I actually had were allergies, but nobody knew anything about allergies and colds in those days; so instead of going to the usual neighborhood public school, I was sent to a somewhat experimental school that was attached to the San Francisco State Teachers College, which was a very progressive John Dewey school. San Francisco State Teachers College is now San Francisco State University. In those days individual kids could work very much at their own speed, which of course fitted me because I was absent a lot. So I worked very much at my own speed. The reason I bring this up is that – since it was attached to the state teachers college – one day, a couple of professors of music came down and said, "Does anybody want to learn the violin?" I was about nine years old and I guess I was in the third grade – actually I was pretty bright so I guess I was in the fourth grade. I raised my hand and said, "Yeah, I'd like to learn." When I announced this at home, my father was pretty well scandalized.

Farreras: What did he do?

Handlon: He was a minor official with the local street railway, and he didn't have much use for musicians. Anyway, I persuaded them that I wanted to learn to play the violin, and so they finally got me a violin and I took private lessons. So I started out playing the violin, and not terribly well. Anyway, the reason that that's of some importance is that by the time I got into high school, I was playing in some amateur orchestras, and they would always say, "If we only had an oboe player." So I decided that the way to get out of playing second violin and not getting anywhere very fast was to learn to play the oboe. So at about 12 or 13 years of age, I again went to my dad and said, "Hey, I'd love to play the oboe, besides, it would be good for my asthma!" Again, after much moaning and groaning, he acquiesced. This was instant success as far as I was concerned. By this time I was in regular public school and was going to high school and doing well academically, and even better as far as music went. By the time I had to graduate from high school, I was doing a lot of music, and the last place I ever wanted to go to was college. I was taking a lot of private lessons, learning composition and conducting, and by the time I was 18 years old I was a professional musician, had joined the union and was doing a lot of radio work and some symphonic work, playing extra oboe with the San Francisco Symphony. I had actually gone back to Juilliard and didn't like that, and, for some personal reasons as well, came home. So I actually didn't go to college immediately. I started at Berkeley, as a music major, until I was about 23 years old.

One of the things that had happened to me was that I was beginning to get increasing performance anxiety playing, and this got me wondering about psychology. This performance anxiety climaxed in a radio performance in which I had to play a not-terribly-important solo in which I completely broke down and had to stop in the middle while playing over the radio. It was one of the most traumatic events in my life. I had actually started going to Berkeley as a music major, trying to go to college part-time – the Second World War had already started – and trying to maintain my professional playing. After this event, I decided that this was not going to work and I had to make up my mind: was I seriously going to college or was I going to try to maintain my professional playing? This experience on the radio made me decide I'd better think seriously about college. I decided to move away from the Bay Area and the temptations of music and move South to UCLA. There I switched from a music to a psychology major.

Let me just add some background. My mother had gone to college here in Santa Barbara and was a home-economics major, and my dad – who came from quite poor parents – had not finished the sixth grade. But he was a well self-educated person, very much interested in literature and very much appreciative of men he'd met in his work life who were educated and knew literature. So as his son, he saw to it – particularly living in San Francisco – that there were lots of opportunities for me to go to the theater, plays, operas, and musical comedies. I say all this because going to college was really my idea, I certainly wasn't forced to go to college. Because I had been a professional musician and had gotten some very good jobs. For example, during the World's Fair in San Francisco in 1939-1940, I had played with the official band and had been able – because I lived at home – to sock away a good deal of money. So when I finally went to UCLA, I was able to afford the move and be very much on my own until I could get settled.

Farreras: Why not stay at Berkeley and do the psychology major there?

Handlon: Well, I would have been too tempted to take a music job if I stayed in the Bay area. The only way I could get out of the temptation would be to move away. That's why I went to UCLA. Besides, I very much liked the relaxed Los Angeles atmosphere. There was something about UCLA which I really enjoyed. That's why I did that, to get away from that San Francisco pattern. I was beginning to fight at home. Whereas my dad was very supportive, he was also an extremely controlling father, and this was my way of rebelling. I had been an extremely passive kid, my mother was very passive, and I suddenly rebelled in my late teenage years. I just had to get away and begin to live my own life, so that's how I ended up at UCLA majoring in psychology. I had thought psychology had mostly to do with humans. One of my big disappointments in taking my first psychology course at Berkeley was that it seemed to be mostly about the ear and the eye and sensory psychology. It wasn't talking about humans as a whole entity. It was also talking about rats and mazes, not the human as a totality. In those days, which would be in the mid-40s, personality theory and clinical psychology had really not gotten started yet in academia. It really didn't get going until the end of the Second World War. So that was my big disappointment. But I suddenly realized that if I wanted to be in psychology in those days, one of the ways was to be a learning theorist. I was pretty good at it. I was, of course, very much influenced by the learning theorists of the day.

Farreras: Who was at UCLA at the time?

Handlon: Believe it or not, I was very much influenced by the leading learning theorist – Edward Tolman – who was back at Berkeley, which is why I went back to Berkeley to get my doctorate. Unfortunately – and this an interesting story – he had refused to sign the famous loyalty oath and had been fired. I had delayed going back to Berkeley for a year and by the time I decided to go back he was no longer there, which was a terrible disappointment to me. But my main mentors at UCLA were John Seward, who was a learning theorist – though not a particularly strong Tolmanian – and Howard Gilhausen, who was a strong Gestalt Tolmanian and had a great influence on me.

Farreras: So even though you didn't get to deal with the more human or clinical aspects of the field of psychology at the time, you still decided to pursue a career in psychology?

Handlon: Yes, I was very much involved in it at that time and was very much involved in this battle between the Hullian-Skinnerian school and the more Gestalt-Tolmanian school on the other side and I came down heavily on the Tolmanian side. And by the time I got up to Berkeley, Tolman was back living there but not officially in the department. I got to talk to him a lot and he was very supportive of me. It was Tolman who got me my job at Princeton after I got my degree.

Farreras: I was going to ask you about that in a minute.

If Tolman had already been fired from Berkeley, however, who did you actually do your graduate work with?

Handlon: One of Tolman's protégés, Benbow Ritchie. I did my dissertation with him.

Farreras: What did you work on?

Handlon: Again, running rats. A theoretical dissertation, not successful. I could have gotten the results by flipping a coin. But I had actually published as an undergraduate, which was rather unusual at that time. I was enough of a good student that there was no question about my being worthy of a Ph.D., so there was no trouble about that even though my dissertation was not a success.

Farreras: And it was Tolman who got you the position as instructor at Princeton?

Handlon: Yes, that's correct.

Farreras: How did that come about?

Handlon: Well, the old-boy network of those days. The chair of the department at Princeton was a friend of Tolman's. By the time I got there, however, the chairmanship had changed. But I found Princeton rather precious and after a couple of years I was not terribly happy there and they were not terribly happy with me, and that's one of many reasons why I decided to take this PHS post-doctoral fellowship in Chicago and switch formally into clinical psychology.

Farreras: Was this one of those fellowships that the VA and PHS were giving to encourage training in clinical psychology?

Handlon: That's exactly what it was. PHS offered post-doctoral fellowships for training in clinical psychology. The one I had was based in Chicago and was jointly sponsored by the University of Chicago; University of Illinois (Illinois Neuropsychiatric Institute); and, Michael Reese Hospital. It was a two-year fellowship and I spent the first year at the NPI and the second year I split my time between NPI and Michael Reese.

Farreras: And the switch to clinical was mainly because there was funding available for such training fellowships?

Handlon: I was tired of rat-running and learning theory, and my original interest was in the total human person as I had hoped it would be so this seemed to be a way of doing something that was of more interest to me. Indeed it was. I was delighted to make the switch.

Farreras: Did you spend that summer at NIMH before you went out to Chicago?

Handlon: Yes, that was a transition time. I had some time in which I didn't have a job, and one of my buddies from graduate school at Berkeley, Mike Boomer, had gone to NIMH and was very pleased with it there. He was actually a public health officer, and he said, "Well, why don't you come down? One of the things I'm doing and am enjoying very much is working on the Fritz Redl service. Why don't you just come down and I'll get you a job? It won't be at the Ph.D. level, but it's a job and you can see what it's like down here." He was always interested in trying to recruit me in NIMH, and so I agreed. There were no summer slots on the Redl service open at the Ph.D. level so I worked at the Master's level. That was OK with me since my main purpose was to get some experience doing clinical research and to get a feel for what it was like working at NIMH. Also, I could use a short-term summer job doing something of interest. So it was lots of fun and it was interesting to see how a large service like that operated, which was interdisciplinary with psychiatrists, social workers, recreational therapists, and psychologists. There were six pre-adolescent delinquents that Redl had there. They were in Building 10 before they built the cottage for these kids. I enjoyed working with Wells Goodrich as well. It was a good experience and it gave me a little taste of what it would be like if I were to come and work there after I did my two years of post-doctoral work in Chicago.

Farreras: Was it Goodrich's project or Redl's project?

Handlon: Well, Redl was head of the project, and Wells Goodrich, the psychiatrist, was head of the research aspect of it. And I was one of the boys on the research side, which was interesting. We would take time samples of these kids' interactions in various settings, and we'd then report, rate, and judge the interactions using a particular rating scale. It was quite a big project.

Farreras: And after working on that project you then went to Chicago for two years?

Handlon: That's right. It was very useful for me. I was doing psychotherapy and a lot of psychological testing under the close supervision of a variety of different folks. And with a fair amount of free time, I began my own psychoanalysis there.

Farreras: What type of psychotherapy was common at the time?

Handlon: Well, in Chicago those days, you either were a Rogerian or you were in psychoanalysis, the latter, which I began there four times a week. And, I still had time enough to work on a project for an advertising agency.

Farreras: What did that consist of?

Handlon: Well, one of the things I learned – which was fascinating to me – was the excitement of a big advertising agency with an almost unlimited budget; how they can churn out research within two or three weeks. Now, the question was what kind of advertising pitch they should give a new soap. Something like that. But they wanted to pitch it to women, so how should they do that? So they did a national survey within a couple of weeks with all kinds of interviews; with practically a million-dollar budget, you can do that plus write a report. Large-scale research, fast and dirty. So I was sort of the research consultant on that. It was fun and very well paid.

Farreras: That's very popular today; one of the most lucrative areas for psychologists to go into nowadays.

Handlon: Oh, you bet.

Farreras: So at the end of the two years in Chicago, you came back to NIMH?

Handlon: Yes. I'm sure Mike Boomer was very influential, and Dave Shakow had heard about me and I'm sure I had met him along the way.

Farreras: Had you met him during the summer that you had spent there earlier?

Handlon: I must have. I had a pretty good track record and I certainly had met Morrie Parloff. They were really looking for somebody with my combination of a very strong research background and my recent clinical training.

Farreras: A learning theorist who'd undergone psychoanalysis; that's quite ironic!

Handlon: And two very strong years of clinical training in Chicago, and particularly the Michael Reese training.

Farreras: Were you looking to go back to NIMH or were they recruiting?

Handlon: I got recruited, but that seemed ideal to me. I had also met somebody in Chicago whom I had known as a fellow graduate student in Berkeley, a woman and fellow graduate student. After getting her doctorate, she had gotten a job in Chicago and was my teacher at the NPI, and we fell in love, and I convinced her to come to Washington with me. She got a job at Chestnut Lodge. Marion replaced the psychologist – I'm trying to remember her name – at Chestnut Lodge so that was a good match. So we decided to get married, I decided to take the job there at NIMH and it all worked out.

Farreras: Where were you located, T6?

Handlon: No, we were in Building 10, the big giant building.

Farreras: How did you decide what to work on when you arrived?

Handlon: Dave Shakow pretty much left it up to me to decide what I was interested. But I didn't have any particular strong desire, in the sense that I didn't come here to work on a particular thing that I was interested in.

Farreras: I see. Because with your learning background you might have been able to do work in the Animal Behavior or Perception and Learning Sections, or with your clinical background in Shakow's Section of the Chief...

Handlon: Yes, but I didn't want to have anything more to do with animals. I got started with Morrie [Parloff] when I first came. He had gotten me interested in creativity.

Farreras: He said that David Shakow got him working on creativity..?

Handlon: Well, Morrie was well into creativity by the time I got there. We started out doing that. And everybody spent a lot of time talking about possible projects; they were very generous with their time. For example, I could continue to be in therapy, although instead of straight psychoanalysis it was more the Washington style of psychoanalysis, e.g., minus the couch.

Farreras: What were some of the main differences?

Handlon: Well, it was much more interpersonal and much more influenced by...I'm suddenly blocking on the name...

Farreras: Sullivan?

Handlon: Yes, much more Sullivanian. Thank you. In addition, they were also very generous about letting me see some patients on company time, which was very nice.

Farreras: Was that always the case? For some reason, I think I recall Morrie mentioning that that was something Bob Cohen encouraged or implemented at some point – because he thought it was important if you were going to be doing treatment outcome research – but was not always the case.

Handlon: No, I don't know anything about the history of that. All I know is that that was encouraged and not at all frowned upon, as long as we would tape our sessions. Of course, one of the things that did get started while I was there was the formal taping of a psychoanalysis.

Farreras: Yes, Shakow's Folly.

Handlon: Yes, there was a lot of talk about the fact that there weren't any psychiatrists who were willing to do it, to be taped. There was only a psychologist willing to do that.

Farreras: Paul Bergman?

Handlon: Paul Bergman, yes.

Farreras: I see. I thought he was a psychoanalyst. So he was a psychologist?

Handlon: Well, he was not an M.D., let me put it that way.

Farreras: Is it still the case that you are required to have an M.D. degree?

Handlon: No, not anymore.

Farreras: Oh, that's right, I just remembered Morrie mentioning that he was the first non-M.D. person in the Psychoanalytic Institute here in Washington.

Handlon: You know, that's still an issue, a formal issue in some locations. Anyway, what became interesting to me was the fact that we were all encouraged not to be parochial about working only within our own section, so I linked fairly soon not just with Morrie in the section, but with the group [in the Adult Psychiatry Branch] that was interested in the relationship between family dynamics and schizophrenia.

Handlon: Yes. So I would sit in on some of the therapy sessions with the families. I did a lot of that. Perhaps even doing too much as far as personally writing papers and so forth, but a lot of time spent talking and sitting in on therapy sessions.

Farreras: I just got back from the Archives of the History of American Psychology at Akron, Ohio, where David Shakow's personal papers are located and I can't remember if you were the person involved but there were some letters there going back and forth between David Hamburg and Shakow about whether a psychologist from the Lab could be a co-PI on certain projects. And those involved in the Adult Psychiatry Branch were saying no, that psychologists could not have that same status as the psychiatrists on the project, that they were helping with the project but couldn't be considered co-PIs. It seemed to be a real power struggle between these M.D. psychiatrists and the Ph.D. psychologists. Does that ring a bell? I'm almost positive that you were the individual involved.

Handlon: It might have been an issue that I got into later because I worked with Dave Hamburg on a project having to do with stress and stress hormones. Does that sound familiar?

Farreras: I can't remember what the content of the research was right now but I will double check and get back to you on that.

Handlon: I had been one of the main people who had taken some of the data – because I was within that group and the main person with statistical knowledge. I worked it up with Dave Hamburg's permission, and given some presentations in front of a psychological group. Maybe that's what that was all about.

Farreras: It might be. You mean presented to a psychological group like APA?

Handlon: Yes, APA.

Farreras: I see. Well, I'll go back and look at them again and see if my recollection is accurate.

Handlon: I know there was quite a dustup about this, and I actually went up to Bob Felix at one point. I know that there were some people who'd gotten fairly hot under the collar, and I must say that I was doing this in all innocence.

Farreras: What was Hamburg upset about?

Handlon: Dave wasn't upset. It was some of the other folks that were working with Dave. I used to call the dynamics of our relationship with Dave the maypole dance, because we all thought we had a string attached to the maypole, which was Dave Hamburg, and he was very seductive in a way that each one of us thought we were the favorite child and, I think, always surprised that we weren't. Obviously, some of us were because C. Peter Rosenbaum – a young psychiatrist who was not involved in the stress project that I was – and I were invited to go to Stanford with him. But I must have stepped on somebody's toes because I know there were some strong objections to the fact that I had taken some of this data and worked it up. I didn't say it was my data or that I had done this whole thing. Quite the contrary. But I know that people got fairly hot under the collar.

Farreras: What happened when you went and talked to Felix about it?

Handlon: Well, I only heard about all of this sort of second- and third-hand.

Farreras: Oh, so you weren't actually confronted with any of this.

Handlon: Not directly, no.

Farreras: I see.

Handlon: I just heard rumors about all this.

Farreras: So how did it all end?

Handlon: Well, nothing really happened as far as I could see. There were just rumors flying around. Nobody called me up on the carpet and said, "You should have done this," or something.

Farreras: I see.

Handlon: I had gotten quite close to Dave Hamburg. This goes back to my post-doctoral work in Chicago. He was at Michael Reese Hospital at the time or was connected with it before he came and took over the Adult Psychiatry Branch. He worked quite closely with John Mason, M.D., who was over at Walter Reed. In fact, Mason and his team were doing all the biochemical microanalysis, Dave Hamburg was doing some of the psychiatric work, and I was doing some of the psychological testing analysis. When David Hamburg then went to Stanford and became head of Psychiatry there he offered me a job and that's why I left, because I – and my wife – were keen to get back to California. Anyway, that's jumping ahead.

Farreras: When you say that you were doing testing, do you mean WAISs and WISCs or...?

Handlon: No. On the Hamburg, Mason, et al. project we were interested in natural stress; we studied young students, some of them Bennington students. I'm trying to remember the name of the religious group of young folks some of whom were conscientious objectors and came and lived in Building 10 (they were Mennonites and Church of the Brethren). When they moved in we would take blood and urine samples to see what kind of natural-occurring stress would affect their stress hormones. These were kids from the Midwest who were from fundamentalist groups, literally off the farm, who'd come out to the D.C. area to live in a metropolitan setting. For some it was a coeducational living arrangement for the first time in their lives. It was a very different situation for all and obviously meant they were under considerable psychological stress. We were interested in the effect on some of these internal hormones.

Farreras: I see, so more physiological types of assessments.

Handlon: Yes. And we'd give them, at the same time, a daily test of psychological affect, fairly benign "I'm feeling excited, depressed," etc.

Farreras: Self-report tests?

Handlon: Self-report basically, and correlating changes in daily self-reports with changes in the stress hormones as found in their urine and blood samples.

Farreras: I remember Morrie mentioning that psychiatrists expected that the psychologists would do a lot of testing for them and that at some point Shakow decided he'd had enough of that, that psychologists were here to do research, like everybody else, and were not going to be doing any more testing [i.e., service work] for all these psychiatrists in the other sections or labs or branches. Did that happen after you were here?

Handlon: I don't remember that as being a big, big issue, although I could see why that might be the case. That had become the traditional role in a clinical setting. As a matter of fact, that was somewhat of an issue in my post-doctoral training. There was one setting at Michael Reese Hospital where no psychologist was allowed to do any psychotherapy. They were only allowed to give Rorschachs, TATs and so forth. In another setting, where I got supervised by my future wife – who is no longer my wife – by the way, that was permitted. So there was a considerable struggle along that line, whether psychologists were "nothing but" testers, whether they could be permitted to do therapy.

Farreras: And that was already the mid-50s, which is pretty late considering that World War II forced a situation where psychologists had to be involved in psychotherapy because there weren't enough psychiatrists to handle such a large load of psychiatric casualties.

Handlon: True.

Farreras: So we're talking 10 years later, and that's still an issue.

Handlon: Oh, yes. So I can believe that that struggle was still going on. But the fact that Mike Boomer and I and other psychologists at NIMH were, as far as Dave Shakow and others like Bob Cohen were concerned, encouraged and permitted to do psychotherapy, because other psychiatrists who were there who said we were supposed to do a lot of testing for them, these old traditions still going on.

Farreras: I see, but they were okay with all of you seeing patients and doing therapy?

Handlon: I guess they were okay with it, but they also thought that we should be doing all the assessments for them as well; that we should be in our old role as well.

Farreras: And when you mentioned that you were encouraged to work with people from other sections and labs, was that encouraged by Shakow or above Shakow, like, Cohen or Kety?

Handlon: That never was very clear. I was glad to do that because this was all new for me. In my new life as a clinician, I was delighted to be able to rub elbows with all these people, and they were very nice and encouraging to me so that was great. I was very pleased with that. In some ways that paid off and in some ways I sort of felt...I was going to say I felt like a second-class citizen but not really – I felt that more when I went out to teach at Stanford than I did at NIH.

Farreras: Second-class citizen vis-a-vis the psychiatrists, the M.D.'s?

Handlon: Yes.

Farreras: I see. I recall someone talking about how Shakow really encouraged psychologists to work with psychiatrists because the psychiatrists didn't have the background in research and statistics, and psychologists therefore had to be the ones to teach them and rescue them or help them with their research because they weren't able to do it themselves. Does that seem accurate?

Handlon: Yes, there was a certain amount of that. On the other hand, the quality of the psychiatrists there was very high. They were good writers in and of themselves; they would write papers. It just wasn't, as we used to call it, hard-ass research. So there was always a struggle around that.

Farreras: It sounds like you were looking forward to going to work at NIMH. When I spoke with Bob Cohen, he talked about how hard it was to recruit people because the salaries were not as high as those offered in academia as well as because of the stigma surrounding working for the government, especially during the McCarthy era. You had earlier mentioned Tolman refusing to sign the loyalty oath at U.C.Berkeley and whether they were going to require scientists to sign loyalty oaths here at NIH was also an issue. Were you exposed to any of that?

Handlon: I didn't experience that part. That was a very big struggle for me when I had to go as a graduate student to Berkeley, because here was my hero, who didn't sign the loyalty oath, and was I going to sign it or not sign it? I had to do a lot of soul-searching around that. But I didn't experience that while working at NIMH.

What I did experience was this funny phenomenon of salaries all being set. The struggle was not over salaries; it was over space. Competition got expressed in terms of how much space you could command, not over how much money you could command, since that was set. So your status depended on how much space you got, how big a staff you had, if you had a secretary and an assistant, if you shared your secretary, if your secretary had his/her own space...you know, all of that nonsense.

Farreras: I didn't realize that the students at Berkeley also had to sign loyalty oaths. I thought it was only for faculty.

Handlon: No, this included teaching and research assistants. That was a tough one for me.

Farreras: But then you went back when Hamburg offered you a job there?

Handlon: Yes, but I went back to Stanford where, because it was a private institution, I didn't have to sign anything.

Farreras: When you left for Stanford what were some of the things you missed from NIH? What would say were the strengths and weaknesses of a research career at NIH?

Handlon: One of the things I really missed and one of the reasons why I finally left NIMH was teaching. I'm a good teacher. I enjoy teaching.

Farreras: Even though your Princeton experience was not very positive?

Handlon: Well, Princeton was such a crazy place. I grew up in the typical state university and when I got to Princeton I was not used to the gentlemanly scene, and Princeton was a real caricature back in the early 50s, hopefully a little better now. That was a crazy place, but I won't get into that. But I was glad to get to a situation where I could do some teaching. And there was a drifting quality to NIMH. Maybe it had to do with a lack of direction that the place seemed to have or maybe it was Dave Shakow's fault that it was almost too laissez-faire. I wish we had had more direction somehow. It was do what you want to do and I thought there was a lot of wasted time. Some of it was my own fault for not having clear in my own mind what I wanted to do, but it just didn't seem to have much spirit or thrust.

Farreras: Do you mean NIMH in general or the laboratory in particular?

Handlon: I think mainly the laboratory. There didn't seem to be a general spirit about the place. Dave Hamburg's outfit at least had some thrust to it, that particular project. And Lyman Wynne had some certain notion that there was a relationship between family dynamics and schizophrenia, but even that seemed awfully vague and somewhat wishy-washy.

Farreras: So you missed a strong research program that had a specific direction.

Handlon: Yes.

Farreras: Would you say that the other sections had a more direct...?

Handlon: I think, by the nature of what they were doing, probably yes. And I think the complexity of what we were into was part of the problem. There did seem to be some fuzziness about it all. I don't know whether we were trying to do too much at one time or whether it was just the nature of the beast, but there seemed to be a lack of thrust, which was too bad, I think.

Farreras: Well, I don't know if there's anything else that you want to mention about your years here, either about particular people or certain research that you were working on.

Handlon: No. If there's anything that I think of suddenly in the middle of the night that I should have told you I certainly will.

Farreras: Good. As I mentioned to you earlier, I will send you a hard copy of the interview, and then you can add anything that you want to develop or build upon. I'll also look through those Shakow papers I mentioned and send you a copy of them.<sup>[1]</sup> Well, good, thank you so much.

Handlon: Not at all. This was fun to do.

Farreras: I'm glad you enjoyed it. It's very helpful and informative for me.

Handlon: Okay, well, I'll wait to hear from you.

Farreras: Okay, thanks again.

Handlon: Bye.

*End of Interview*

---

[1] It is quite clear from the material you sent me that I was, indeed, the person involved in the controversy around the two presentations (one of them invited) of mine regarding the findings from psychoendocrine stress project. With the hindsight of 41 years, I can see that I was a bit naïve in thinking that, because I was the sole psychologist who had been in on the project from its early planning stages, this gave me equal status with the M.D.s. Therefore, I might be considered to be one of its so-called "Principle Investigators." It was also unfortunate that during the climax of the controversy, Dr. David Hamburg, the former Head of the Adult Psychiatry Branch, had already departed for Stanford and was not around NIMH in person to support my position. Also, with hindsight, this all seems like such a tempest-in-a-teapot, with the all-important bottom line – the actual empirical findings – completely neglected midst all the petty professional squabbles (personal communication, 9/3/2002).